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The making of sociology 2 Developments

Ronald Fletcher

The Making of Sociology

A Study of Sociological Theory

Ronald Fletcher

VOLUME TWO

Developments

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To Rom
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Some Fallacies and Errors of Assessment

I said at the end of Volume 1 that after our study of these early twentieth century developments, we would be able to clarify and throw into the wastepaper-basket—decidedly and confidently—a score of fallacies which were ruining the contemporary perspectives of the subject and breeding misunderstandings. We are now, happily, in that position—the only slight difference being that if we were quite literally to treat each error separately, they would number far more than a score! For convenience, however, we will group them here and there. Some of these errors are more serious than others, but all are of quite basic importance. Here we will do no more than *list* them—in as simple a statement as is possible. It will be seen, however, that they have been fully established by all our studies so far, and that, in this form, they will provide a clear and firm basis for our critical assessment, in Volume 3, of those contributions to the making of the subject which have been made between the second world war and the present day, and which have been claimed as advancements.

Some of this contemporary work is undoubtedly of positive worth, but I shall argue that much of it is a hectically coloured cloud of professionalization (really a concomitant of the institutionalized superficiality of the modern educational scene), a coloured dust not only in the eyes of beholders (I mean some educationalists, social workers and administrators, some window-dressing Vice-Chancellors, and the like, who have their own dust of professionalization in their eyes and who are really only after the dust which gives the brightest glitter), but also in the eyes of many sociologists who think that Scientific Sociology is something which sailed across the Atlantic in distinctive American regalia after the war, with Talcott Parsons like some moving figure-head in the prow—declaiming his 'General Theory' in passages, as Noel Annan once put it, 'of *rare* lucidity'. In Volume 3 we shall examine this star-spangled banner—to which so many disciples have flocked—and try to make clear, and assess, the actuality which is holding it up.

Meanwhile, however, and in preparation for these engagements to come: the list of errors.

Error 1: That sociology lacked clear definition until the post-war years

It is an error to think that the definition of sociology—of its

subject-matter, its nature, methods and scope—was vague or unclear until the post-war period. For those who would *read*, it had been clearly defined from its foundation onwards, and developed consistently within this definition.

Error 2: That it was a new post-war clarification to define sociology in terms of 'social systems'

It is an error to think that the clear definition of sociology waited upon certain post-war efforts especially attendant upon a new illumination that the central and distinctive concern of the subject was *the analysis of social systems*. This had always been plainly understood, explicitly stated, and had never varied. In all the work of the nineteenth century founders, and in that of all those who developed the subject during the early part of the present century, the definition of sociology was perfectly clear. *Sociology was the scientific study of social systems*. This it was in Comte's earliest formulation. This it remained throughout.

Error 3: That the making of sociology up to the second world war was a creation of separate, distinct and divergent 'schools'

It is an error to think that those who contributed to the foundation and development of sociology up to the second world war set up distinctive 'schools' which were separate and 'divergent'. There never did exist an 'Evolutionary' school, a 'Functionalist' school, a 'Verstehen' school, a 'Sociologistic' school, a 'Psychologistic' school, an 'Instinctivist' school, and the like. *All* the sociologists we have studied were evolutionists. All of them accepted structural-functional analysis. All of them (though with Durkheim inconsistently) accepted the understanding of action in terms of meaning as well as the objective study of phenomenal events. All of them recognized the distinctive level of 'associational facts'. None of them were psychological 'reductionists'. All of them accepted the place of instinct in human nature and in the process of institutionalization. Furthermore, none of these elements were *separated* from the others in their systems of ideas. They were all seen, in their interconnection, as parts of *sociology as a whole*. *Schools* of sociology are the *inventions of teachers or text-book writers*. They are *false* and distorting divisions.

Error 4: That the progress of sociology lay in the 'convergence' of these 'schools'

It is an error to think that the progress towards the clarification of sociology *as a whole* took the form of a latter-day coming together of

separate 'schools'. This is a false and harmful perspective. There had always been a clear and comprehensive definition of *sociological analysis as a whole*, and the developments we have studied were all of important *dimensions* within it, and were not such as to deny the importance of *other* dimensions. The correct perspective, as we have seen, is that a 'conspectus' of the nature of sociology had been clarified by the end of the nineteenth century, and that the efforts of the few following decades were a *deepening* of each dimension within it, a clarification of the concepts and methodological issues appropriate to each dimension, but *always* within the context of the whole. The perspective was one of a deepening and enriching of all the components of an agreed conspectus, *not* a latter-day coming together of disconnected bits. Certainly *agreements* became more richly evident—as, for example, *on the nature of the process of institutionalization*; and certainly the connections and interrelationships *between* the dimensions were made more clear—as, for example, in Mead's linking of the elements of psychology, meaning, observable 'act', and structured institutions; but these were illuminations of the same agreed subject-matter resulting from the exploration of its several dimensions. It was sociological analysis *as a whole*, *as a unity*, and *in all its dimensions*, which was being substantially developed within its own defined nature—not a latter-day dragging together of disparate 'schools' in terms of an awareness of their connections not previously seen.

The harmfulness of the *false* perspective is that it can lead to the *ignoring* of many aspects of supposed 'schools' (the 'evolutionary', 'functionalist', 'instinctivist' schools, etc.) which are not thought to be relevant to the supposed 'convergence'; and this can lead to an impoverishing of the subject if all these supposed 'schools' were really developments of essential elements of the whole. It can lead, in short, to a 'selective perception' of the most narrowing kind, and—worse—the setting up of a *doctrine* rather than the careful, indeed jealous, maintenance of a rich many-faceted subject within which all dimensions can be freely studied.

Error 5: That 'Positivism' was a 'school' limiting sociology to the concepts and methods of the 'natural sciences'

It is an error to think that 'Positivism' was a school or even an approach to sociology clearly distinguishable from others by its insistence that its *subject-matter* was completely like that of the natural sciences (i.e. consisting of *phenomenal events*) and that the methods it should pursue and the 'laws' it could establish—to be truly scientific—must be confined to those of the natural sciences. *All* the sociologists we have studied were *positivists* in the straight-

forward (and only valid) sense that they recognized and observed the requirements of positive science for the production of testable knowledge: but this had *never* stipulated only the methods of the natural sciences. Even at the outset, Comte's statement of 'positive science' was such as *especially to emphasize* that each science studied a *distinctive level of subject-matter* which required *distinctive methods* and established 'laws' (statements of regular interconnection) *at a distinctive level*. His statement also especially emphasized those aspects of both subject-matter and method which *distinguished sociology from the other sciences*, and Comte's own 'types' of society, constructing his 'rational fiction', were quite explicitly orders of '*Feeling, Thinking, and Acting*'. The identification of 'Positivism' with a narrow 'natural-science' orientation to the study of social facts as if they were only 'phenomena', is therefore a false and distorting caricature, and to set it up as a 'school' *opposed* to, or in *conflict* with, Max Weber's kind of subjective understanding of social *action*, and as denying the concepts and methods appropriate to such interpretation, is obvious nonsense.

Error 6: That 'Organicism', similarly, was a distinguishable school of sociology

It is a similar error to think that there was some separable school of sociology especially insisting that society was an organism indistinguishable from a biological organism, and to be observed only in terms of 'phenomenal events' and not at all in terms of social *action*. *All* the sociologists we have studied were completely clear that they were studying *social* systems, and though occasional thinkers—like Pareto—used the analogy with *mechanical systems* for specific purposes, they *all* (including him) used the *organic analogy* to indicate what they meant by the 'consensus' of social institutions. But *no* theorist equated society with a *biological organism*.

It is true that Durkheim in many ways came close to conceptual extremes of this kind (i.e. of both 'positivism' and 'organicism' as falsely conceived), and that Radcliffe-Brown followed him in this. But even Durkheim was clearly insistent that *social systems* were a level of actuality quite distinct from *organic systems*, and defined 'social facts' as '*ways of acting*'. And we have seen that his emphasis resulted really from his efforts to find *observable indices* for the inner facts of sentiments and values, and to make clear the *associational level of human psychology*. He was guilty of ambiguity, but not an intransigent 'organicism' when all his ideas are laid bare.*

* Even if Durkheim and Radcliffe-Brown were themselves prone to narrowness, it is obviously an error to use terms with connotations only appropriate to them. 'Positivism', as a statement of the nature of science, is far older and wider than one limited conception of it.

To link 'positivism' falsely conceived with 'organicism' falsely conceived and to set them up as a separate conceptual position (e.g. 'positivistic organicism') which ignored other dimensions of the subject—such as the subjective understanding of *action*—is therefore not only a nonsensical thing to do, it is an outright misconception and misrepresentation of what was, in fact, the case in the founding and development of sociology.

Error 7: That 'Functionalism' or 'Structural-Functional Analysis' was ever a separate school or theoretical position in sociology

It is also an error to think that 'Functionalism' ever existed as a separate or even special position in sociology (or in social anthropology) before the time of the second world war. It is even an error to think that Social Anthropology was founded upon it as a basis. It is an error to think that 'structural-functional analysis' was ever a mode of studying social systems separated or disconnected from all the other dimensions of sociological analysis. It is also an error to think that 'structural-functional analysis' is, or ever was, to be *identified* with sociological analysis as a whole. Sociological analysis always *contained* this and much more besides.

Error 8: That 'Structural-Functional Analysis' assumed a functional harmony and integration of social institutions and did not take account of conflict in society

It is an error to think that 'structural-functional analysis' ever postulated or assumed something to be called a close 'harmony' or 'integration' among social institutions. Social 'forms' clearly existed and were inter-connected in their origins, operations, changes and developments, in society, and it was certainly supposed that there were 'strains to consistency' and 'strains to harmony' among them. Structural-functional analysis was considered necessary for investigating this interconnected network of institutions—this distinctive kind of 'consensus'—but it was *always* recognized that the activities of men in society were characterized by *conflict* as well as *co-operation*, and that institutions and their values took shape to deal with, and to *regulate*, both. It is an error, therefore, to think that 'functional theory' ever assumed only co-operation and harmonious adjustment, and that a separate kind of theory—'conflict theory'—was necessary to introduce greater realism into its appreciation of social actuality. 'Functional theory' and 'Conflict theory' are not opposites. This is a confusion. It is true that Simmel showed that even conflict contributed to associational bonds, but this did not in any sense deny or suggest a diminished conception of the

power of tendencies either to conflict or co-operation in society, and we have seen that the emphasis upon *both* conflict *and* co-operation was a common strand running throughout the work of all theorists from the very beginning of the subject.

Error 9: That 'Structural-Functional Analysis' was a 'static' theory of society, and not applicable to studies of social change

It is an error to juxtapose 'functional analysis' and 'theories of social change' as though they had nothing to do with each other, and were mutually incompatible. Structural-Functional analysis was *never* conceived as a mode of analysing a society in a *static* condition, or in a condition of harmony or integration, within which no causes of social change could arise. Neither were systematic studies of social change ever undertaken without structural-functional analysis. It is false to think that some *other* element of theory—'conflict theory' or some other 'dynamic' theory of change—was ever required to render structural-functional analysis *capable* of examining social change. *All* those who accepted structural-functional analysis also *insisted* that societies were *essentially* historical, changing processes, *always* experiencing conflict as well as co-operation. Indeed, all the major theories of social change and social evolution were in fact produced by theorists who insisted on the employment of the structural-functional method (as well as other methods) of sociological analysis. Comte, Spencer, Marx, Ward, Sumner, Giddings, Tönnies, Hobhouse, Durkheim, Weber, Cooley, Pareto, Malinowski, Radcliffe-Brown . . . it is an impressive list. The problem is, really, to find a theory of social change which did *not* utilize structural-functional analysis; and a structural-functional analysis of society which *denied* that society was a process of change. It is perhaps worthwhile especially to emphasize that Malinowski and Radcliffe-Brown—often thought of as 'Functionalists'—were explicitly clear about this, and were like everybody else. They made it perfectly plain that a 'functional' analysis of society was in no way in conflict with theories of evolution or change.

Error 10: That Social Statics *studied a* stationary *condition of society, and that Social* Dynamics *studied society in a condition of change*

It is an error to think that the terms 'statics' and 'dynamics' referred in any sense to a *stationary* or *mobile* condition of society. To think of structural-functional analysis as a major element of 'social statics' in *this* sense is what Ward had called 'the Fallacy of the Stationary'. *No sociological theorist ever maintained that society was*

ever stationary and that structural-functional analysis was particularly appropriate to the study of such a social condition. Structural-functional analysis was certainly held, and properly, to be an important element of '*Social Statics*'—but this was that part of sociological method concerned with the analysis of the distinctive nature of its subject-matter: i.e. *social systems*: and social systems were held to be (among other things) essentially of a historical nature; essentially *processes* of *change*. Similarly, 'dynamics' was *not* the study of the *static* social system now in *movement*. '*Dynamics*' was that detailed study of the variety of societies actually existing in the world of history, which utilized the analysis of the distinctive nature of social systems which '*Statics* had provided.

This error is therefore simply a confusion about the meaning of terms.

We may note that even Radcliffe-Brown, who tried to be more meticulous than most in this, and extended his conception to distinguish (additionally) between 'Synchronic' and 'Diachronic' studies, was nonetheless insistent that these were *both* studies of *change*: one—of the processes whereby the activities of a social system *sustained its form* (though experiencing continuous changes within it), and one—whereby the activities of a social system led to a *change in its form* (so that it became a different type of society).

This matter was therefore perfectly clear. To identify structural-functional analysis with the *static* study of a society and to think it inapplicable in explanations of social change, is a mistake. Structural-functional analysis was always one important element in the entire conspectus of sociological analysis which studied social systems as being (among other things) *essentially* processes of change.

Error 11: That 'Structural-Functional analysis' was separate from, and in conflict with 'Evolutionary Theory'

It is an error to think that structural-functional analysis was ever thought of as being opposed to (or incompatible with) historical studies and explanations, and, more specifically, to theories of social evolution. *All* the theorists who *explicitly* put forward theories of social evolution—Spencer, Hobhouse, Durkheim, Weber, and others—*explicitly* used structural-functional analysis. Also, those who especially wished to use structural-functional analysis as a basis for clearly formulated empirical studies—Malinowski and Radcliffe-Brown—took pains to make it *explicitly* clear that they *accepted* evolutionary theory, and that their concentration upon the use of the one did not deny their acceptance of the other. There was never any separation or conflict here.

Error 12: That there was a distinguishable school of 'Evolutionary Sociology' from which specific elements of theory had to win their freedom

It is an error to think that 'evolutionary sociology' ever existed as a particular kind of sociology, well-defined in separation from other elements of theory. *All* sociology was *evolutionary*, in the sense that it studied social systems within the wider context of the evolutionary perspective, and in the sense that concepts of social evolution formed an essential part of its analysis. Every theorist we have considered accepted, included, and made use of, 'evolutionary' concepts. However, we have seen it to be a vast over-simplification to think that there ever was *one* such thing as 'evolutionary theory' in sociology. Many things were studied under this heading: the implications of biological evolution for the clarification of 'functional requisites' and for the study of the process of institutionalization; the study of 'origins' in the sense of basic, universal 'causes'; the study of patterns of institutional change—of the differentiation and specialization of structure and function, etc., the study of mind and knowledge, their growing powers of control, and their influences upon directions of institutional change; these and many other questions were involved.

Furthermore, structural-functional analysis, the understanding of social action, the construction of 'types', the explicit use of certain psychological assumptions were all involved in these many kinds of 'evolutionary theory'. To think of these elements as winning their freedom from 'evolutionary sociology' is therefore a wrong perspective.

The correct perspective, again, is really that of *sociological analysis as such* having been given a clear initial statement which incorporated *all* these components in interrelationship with each other—being deepened as each element became more richly developed, and as their implications of method became more elaborately worked out. In fact, as we have seen, *none* of the distinguishable components of sociological analysis *did* free themselves from 'evolutionary theory', nor did they want to. They still accepted it and worked within its context and with its assumptions. Evolutionary theory was never abandoned in sociology.

Error 13: That evolutionary theories were monocausal

It is an error to think that any of the major theories of social evolution were 'monocausal'. Spencer, Westermarck, Hobhouse, Durkheim, Weber, and others all introduced many causal factors into their theories. To think otherwise is simply not to know their work.

Hobhouse, for example, stressed a central *feature* of evolution—the growing power of mind to control the natural environment, society, and the conditions for its own action—but a multiplicity of factors were considered in relation to this. The same is true of 'typological' accounts of specific (though large-scale) configurations of change. Tönnies, for example, emphasized the *one* dichotomy between 'natural' and 'rational' will—but, of course, a wide range of factors was drawn into this deliberately sharpened *interpretive scheme*.

Error 14: That the evolutionary theories traced historical 'origins'

It is an error to think that evolutionary theories traced 'origins' in the sense of seeking to uncover original historical events, and then hoping to explain subsequent social developments in these terms. They were seeking *basic elements of causation*. But though it was thought that these could account for the *common existence* and the *persistence* of certain social requisites and patterns of institutionalization, it was certainly never thought that they could explain all the historical complexities of social change. The essential thing, however, is that this was one approach to the clarification of the elements of causation underlying social institutions, it was not a business of historical chronology.

Error 15: That the evolutionary theories were 'unilinear' and 'historicist'

It is an error to think that any of the major evolutionary theories we have discussed were *unilinear* in the sense of specifying one single line of social development through which all societies, necessarily, had to pass. Sometimes Spencer wrote in ways that seemed to suggest this—but a full study of his work shows quite clearly that he explicitly guarded against it. He clarified 'types' of society and sequences of change from one to another, but his propositions were always of an 'If ... then ...' nature. *If* conditions constraining a society towards militancy increased, *then* the social structure would change to approximate to a certain 'type'. But Hobhouse and others guarded themselves so explicitly against this error as to make the criticism a mere absurdity. No *inevitability* of sequence from type to type of society was claimed and no 'historicist' *prediction* of inevitable future states of society was indulged in. All these theorists sought to clarify these processes of patterned change to provide a basis for *judgment* and *action*; and all emphasized *human responsibility* for the future making of society.

Error 16: That 'Action Theory' as developed by Weber was a new orientation in sociology

It is an error to think that Weber's distinction between *social action*

(which could only be explained by the imputation of meaning) and *phenomenal events* was new. Throughout the founding and development of sociology, the distinctive *teleological* attributes of the human mind in all its practical and social activities had been recognized, and explanations had included elements of choice, deliberation, character, ends sought and means calculated, values and purposes employed, in *social action*. We saw, too, that towards the end of the nineteenth century, in the work of Giddings especially, these matters were stated quite explicitly even to the point of clarifying 'types' of social action almost identical with those developed by Weber. However, the point of this observation is not at all a matter of establishing who said what first.

The essential point is this. It is a great error to suppose that sociology was 'positivistic', 'organicist', 'evolutionary', or whatever other word one might wish to employ to suggest that it was such as completely to ignore (or fail to recognize) this dimension of *action* before it was introduced as a new and distinctive orientation by Weber. This is a false and harmful perspective.

Sociology *always* included these considerations as to the qualitatively distinctive nature of men in society—as a subject-matter for science—and the problems for appropriate and satisfactory methods which this recognition posed. To set an 'organism-environment' kind of sociology and an 'actor-situation' kind of sociology *against* each other, as two distinct and opposed orientations to sociology, is therefore to pose a dichotomy which never existed, and to belie the whole course of the making of sociology. Sociology *always* included *both* these in an entire scheme of analysis which—whilst making proper logical distinctions—had never found it necessary to separate the two.

None of this—it will be readily understood—is at all to deny or to belittle the great worth of Weber's development of this dimension of analysis. It is only to deny the false perspective.

Error 17: That 'Action Theory' was opposed to 'Positivism'

It is an error to suppose that Weber's methods for the subjective understanding of social action were at sharp odds with 'Positivism' as a statement of the nature of science. Positivism had always clearly maintained that each science had to develop its own methods to deal satisfactorily with its own distinctive *level* of subject-matter. Weber elaborated a method of explanation by the disciplined imputation of meaning *exactly to accord with the demands of positive science for a testable causal explanation*. There was no conflict here.

Error 18: *That 'Action Theory' was isolated from, opposed to, or failed to see the relevance of 'Structural-Functional Analysis'*

It is an error to think that 'Action Theory' was put forward by Weber in isolation from all the other elements of sociological analysis. Weber was an evolutionist. He thought that 'collective psychological configurations' were of importance. He thought that structural-functional analysis was *indispensable* as a preliminary basis for the formulation of 'types' of social action as he envisaged them. He accepted the necessity and validity of all the other dimensions of sociology. He simply *concentrated* (as a matter of choice for his own work) upon the subjective interpretation of social action. His rejection of a 'scheme of categories' from which explanations might be deduced was *not* because he failed to see the relevance of structural-functional analysis but because he did not think it a feasible business of science to try to set up 'schemes of categories' sufficient for the statement of all kinds of problems. Sociological concepts and methods were *tools* for getting the investigator closer to grips with the actuality of the concrete configuration of action which he was seeking to understand and explain. And the scientist should be free to pose such problems as he felt to be of cultural significance—not be tied to, and burdened by, a 'categoreal scheme'. A sociologist needed a bag of good tools, not an all-embracing doctrine of postulates and deductive procedures—a kind of casuistry of inference. The skill required in sociology was not primarily that of logic but that of the accurate, incisive imputation of *meaning* at work in concrete configurations of action, together with the ability of marshalling knowledge in such a way as rigorously to test such interpretive explanations. It was *research-oriented*. Theory was a servant to the accurate explanation of empirical fact.

Weber aside—we have also seen that all the other sociologists (again with Durkheim and Radcliffe-Brown as the exceptions) combined the study (and meaningful interpretation) of human activities with structural-functional analysis with no difficulty whatever, and with no sense of any lack of fit.

Error 19: *That Sociology was opposed to Psychology and ignored or excluded it*

It is an error to think that in the founding and development of sociology there was anything but the closest relationship between psychology and sociology. We saw earlier that all the nineteenth century founders of sociology sought a correct relationship between the two subjects. Comte and Mill had clear statements about it at the outset—and very well considered ones; and Ward and Giddings had very clear and elaborate statements about it at the turn of the century. We

have now seen also, however, that all those who developed sociology in the early part of the present century were not only persuaded about the importance of the place of psychology in sociology but were as psychological as the psychologists. Indeed, we have seen that a great contribution was made *within sociology* to *the satisfactory making of psychology*. A detailed way of analysing the associational context which was essential for a satisfactory human psychology was actually established. Durkheim's apparent attack on psychology was, as we have seen, a correct attack on an insufficiently conceived *individual* psychology, and on an incorrect assumption that this could explain *associational facts*. When Durkheim is understood, however, far from demolishing psychology negatively, it could be claimed that he was one of the greatest contributors to the *making* of the subject. What is certain is that there were not only the closest relationships between the two subjects, but that these were now clearly seen to be even closer still. Indeed, the subjects were seen—in important ways—to be *part of each other*.

Error 20: That the psychological emphases in sociology were 'reductionist' or 'psychologistic'

It is an error to think that the considerable pre-occupation with the psychological aspects of institutionalization on the part of some theorists was *ever* of a *reductionist* nature. *No* theorist we have considered (not even McDougall or Freud) thought that associational processes could be explained by some more basic 'individual psychology'. On the contrary *all* of them thought that the distinctively human qualities of the individual 'self' were the outcome of association, and inseparable from the establishment of social 'sentiments', values, ideals, etc. We saw earlier that Mill was not guilty of the supposed error of 'psychologism', as contrasted with other theories such as that of Marx. Now we have seen that all the twentieth century theorists were even more plainly free from this error. The striking truth is that these men, in the making of sociology, established the only satisfactory basis for a human psychology. Or perhaps it might be better to say that the making of sociology and a satisfactory psychology of man were all of a piece.

To think that any of these men—Westermarck, for example—was a 'psychological reductionist' as against others—say Durkheim—who was not, is, as we have plainly seen, a simple error. The positions of these men were far more in agreement than disagreement, and both of them, indeed *all* the theorists we have studied, took their stand on their conviction concerning the *psychological creativity of association*. They were all solidly agreed on what we can make our final statement on this: that it is an error to think that a satisfactory human psycho-

logy is *possible* independent of the context of association and communication.

Perhaps it is worth a note, too, that there was no such group as a group of 'psychological sociologists' distinguishable from the rest. Our studies have clearly demonstrated that *all* the sociologists of the early twentieth century included the *same* psychological elements. They *all* emphasized the importance of them and incorporated them in a wider scheme of analysis. Sociological analysis was *a whole*, and *included* psychological elements among the other essential elements in its entirety.

Error 21: *That sociology 'in the grand manner' was not 'empirical'*

It is an error to think that the large-scale theoretical work of those who founded and developed sociology, including their large-scale theories of social evolution and social change, were not empirically oriented or empirically based. On the contrary, we have seen that *all* these theoretical developments were efforts to *engage* with the *distinctive actuality* of human society in ways which were as scientifically accurate as possible and such as to do full justice to the intricate nature of the subject-matter. There are some who think of *facts* in society only in terms of numbers of heads, numbers of houses, numbers of water-closets in houses, the scales of wealth and poverty existing among the population, the state of the roads. They think of *empirical* sociology as a process of *getting to know these facts*—by way of social surveys, door-to-door interviews, the collection of statistics, and the like. They thus think that Booth, Rowntree, Le Play, Branford and Geddes, were *empirical*, and the *grand theories* of Westermarck, Hobhouse, Durkheim, Weber, were not.

Now this is a simple error, and it is clear that people who advance this point of view have simply not begun to understand sociology at all. No one—certainly none of the 'grand theorists'—would wish to deny the great importance of discovering how many houses in society lacked piped water, or what the sizes of school classes were. They saw clearly the important place of *sociography*. Indeed Tönnies and Weber gave their attention to quite specific investigations of this kind, and Hobhouse was much concerned with social conditions, wages negotiations, and the like. Furthermore, we have seen that Booth, Rowntree, Geddes, Beatrice Webb, William Beveridge mingled quite happily with men like Graham Wallas, Westermarck and Hobhouse in the early meetings of the British Sociological Society, and listened there to papers from Durkheim and correspondence from Tönnies. There was no problem; no lack of understanding here. But, as we have seen, the theorists who devoted themselves to the development of sociology were concerned with much more pro-

found issues than these; and with conceptual problems of quite a different level. They were concerned to understand the very nature of human nature in society itself: the nature of institionalization, the creativity of human association, the nature and change of social 'forms', the entire development of human societies and the juncture which all these processes appeared to have reached in the significant modern transformation.

But in all this their *entire emphasis* was *empirical*. Their entire effort was towards the production of testable explanations of all the distinctive dimensions of social actuality. And, as we have seen, their entire effort was a movement from *theory* to *theories*, *from* a *conspectus of analysis to* the production of *definitive theories* in defined areas of study, in order to develop the accuracy and sufficiency of the subject. Furthermore, their books were full of a rich array of systematic comparative knowledge, drawn from the best sources they could find, and certainly more wide-ranging and exhaustive *in their factual content* than anything else that had previously been produced. The criticism that their work was not empirical really makes one gasp.

The only conclusion one can reach here is that there are some critics who have not only not studied the literature of sociology, but simply do not have a philosophical grasp of the problems with which it is concerned.

Error 22: *That sociology was not of* Use.

A further serious error follows from this misconception. It is an error to think that sociology was not of *use* in society, or did not aim at being of use, simply because it did not produce a schedule of facts about the bric-à-brac of the social apparatus (e.g. buildings, road and rail services, school milk-services, and the like). This again is an error of *levels*. Sociology as a science was *not* (whatever people might think) simply a way of collecting facts of this kind. It did not aim to be *useful* at this level; though sociographic studies certainly could be. However, at another level, we have seen that nothing was of greater power in the minds of those who devoted themselves to the making of sociology than that the subject should be of use in the vast task of the re-making of institutions in the global transformation of societies in the modern period. All these theorists (Pareto still the one exception) had a humane concern at the heart of their scientific efforts. All of them emphasized the centrality of values and morality in society, the importance of an 'ethic of responsibility' in the approach to social reconstruction, and all of them saw the production of reliable knowledge as a necessary basis for wise judgment and effective action. The fact is that—as from the earliest statement of Comte—all these

theorists saw sociology as that basic science on which the great *political task* of the modern world should rest. Sociology was a necessary (though not the only) basis for the *art* of politics.

It is in this same sense that it is an error to think of sociology as a training for social work or social administration in any simple or limited sense. Social work and social administration are *arts* in society with specific tasks in view. Certainly they could benefit from resting upon an education which included sociology and its perspectives. But to think of sociology as part of a professional training for these tasks is simply misguided. Again, it is a question of the philosophical grasp of what is involved. There are educationalists, social workers and others, who think that sociology (they think the same about psychology and moral philosophy) is something which can be provided for their trainees in courses of a few months duration. This is what I meant earlier by the institutionalized superficiality of the modern educational scene. This sort of thing simply cannot be done. It rests upon misconceptions and sheer impossibilities.

Sociology was certainly made for use, and was demonstrably *of* use, but in a much broader and deeper way than such misconceptions could suppose.

Error 23: That sociology was of more recent development than the other social sciences, and either a 'collection' or a 'residuary legatee' of them

It is an error, as we have now seen with overwhelming evidence, to think that sociology was a relatively late 'newcomer' to the social sciences. From the early nineteenth century to the time of the second world war—almost the mid-twentieth century—the foundation and the development of sociology had been firm, continuous, consistent and clear. The idea that economics, political science, or any other 'special' social science existed before it is simply not true. The men who were supposedly 'economists'—Adam Smith, John Stuart Mill, Marx, Pareto—were in fact participants in the making of an entire science of society within which economic systems of production and exchange formed only a part. It is high time that the myth of the earlier development of the special social sciences was exploded: for myth it is. Similarly, we have seen that it is an error to think that sociology was ever thought of as an over-all 'collection', or 'bringing together' of already existing and clearly defined special social sciences. Another myth ripe for explosion! As is also the idea that sociology was the 'residuary legatee' of the other social sciences: that political science studied the political aspects of society, economics the economic aspects of society (and so on . . .), and that sociology was then necessary to study the *social*—the subject-matter left behind. Really! When you consider the views of this kind which have been seriously

advanced by professors of social science, you begin to believe that 'Academia' is a home for the mentally retarded!

At any rate, we have seen that the nature and scope of sociology was clearly defined and established during the nineteenth century, extensively developed in all its dimensions during the earlier part of the twentieth, and that by the time of the second world war its nature and range of application was very well founded. Indeed, we have seen that, far from *deriving* from other human sciences, its nature was very powerfully influencing the growth and formulation of others. The improved formulation of psychology and anthropology, for example, owed as much to the influence of sociology as they did to that of biology.

The truth is that sociology *was not conspicuous in the universities*, and it has been thought to be a newcomer among the social sciences simply because, after the second world war, it *did* become a subject in the universities. But this was *not* a measure of the existence of a subject, but of the conservatism and intransigence of the universities—as well, no doubt, as of a mere lack of endowment. Universities are places in which the defence of entrenched interests is only moderated by the provision of attractive endowments. A subject which brings money is respectable. This is why the dust that gives the brightest glitter matters. Institutionalized education has not had much to do with the making of new subjects; much more with the stereo-typing of them. Creative thought is put into the hands of teachers and administrators, for whom the organization of it becomes a career.

One important and forceful aspect of this point about these conceptions of the relations between the social sciences lies in this fact that such supposed divisions are not only intellectual considerations, but grounds for the distribution of resources in the universities. They are grounds for academic intrigue and war. And never has this war been waged with more virulence than now. All the comments which Professor Wenley made in 1906 to the British Sociological Society about the problems of establishing sociology as a university subject could be made in the present academic scene—and with far deeper truth than he could then have realized.

Error 24: That social anthropology exists as a subject distinct from sociology

One matter of the supposed relationship between social sciences deserves special mention—though this can be brief. It is an error to think that Social Anthropology is, or ever was created as a subject distinct from Sociology. What came to be called Social Anthropology was quite straightforwardly the application of Sociology in Anthropology. It was the use of sociology in the study of the social systems

of primitive (non-literate) peoples. There are not two subjects, but one.

Error 25: That the scientific and professional development of the subject meant that sociology became 'value-free'

It is an error to think that the achievement of a clear distinction between philosophical ethics and the scientific study of social fact, led to any change whatever in the moral and ideological commitment of sociology, and a situation in which sociology was 'value-free'. We have seen that there were many important elements in this complex issue, and they cannot be reiterated here. It was clear, however, that the extent to which *social fact* could be *scientifically known* by an accurate and sufficient *imputation of meaning* without an explicit philosophical clarification of *ethical criteria* (such as 'justice'), was shown to be highly dubious. It was clearly agreed that ethical criteria were increasingly important in the *actual nature* of social facts, and in the deliberately brought about changes of them; and that an understanding of moral purpose entered necessarily into any explanation of them. It was clearly agreed that the selection of scientific problems and (in Weber's sense) the construction of the 'object of knowledge' could not be value-free (and, indeed, that such an ethical neutrality was not demanded by science). And it was also agreed that the scientific rigour of study independent of the values involved in selecting a problem, was itself a commitment to the impartial pursuit of truth—itself an important value. In all this activity of the most searching kind for accuracy of criteria in seeking reliable knowledge and judgment, it was also clear that sociology had necessarily to take issue with all other theories which ignored such standards of criticism and test. Sociology therefore inescapably carried an ideology of its own in engaging with other ideologies.

All this aside, we have seen that almost all the theorists we have studied—whether prone to ambiguities or not—rejected any simple-minded notion of ethical 'relativism'. They advocated and adopted a position of moral responsibility. Their position was that which, from the beginning, had been rooted in Comte and upheld by all the thinkers of the nineteenth century. They believed that a great transformation of human society was afoot; that it promised much for human dignity and welfare, but carried grave threats of de-humanization. They believed that a satisfactory resolution of its many aspects required responsible judgment and deliberate action; that—for this—the most exact knowledge was required as a basis, and that no reliance upon traditional beliefs and values, on an 'ethic of inspiration', was any longer sufficient. An ethic of 'responsibility'* of 'realistic

* Weber.

humanitarianism'* no longer worked out in the arm-chair in the study, but in the thick of the study of complex social facts,† was required—indeed, was necessary. And such an ethic they tried both to provide and to uphold.

Whether philosophically correct or not—but we have seen that substantial grounds were given for it—what is certain is that in accomplishing a professional liberalization of sociology and a firm basis of freedom for research-oriented work, the men responsible for these developments did not at all envisage, or advocate, or themselves adopt, any change in the ethical commitments of the subject. The professionalization of the subject was not for the sake of the professionals themselves—but for the accurate and sufficient explanation of problems of cultural significance. And these explanations provided knowledge for judgment and action: for the avoidance of inhuman evils in modern industrial society—narrow, ruthless, contractual expediency; the disorganization of institutions and the disorientation of persons; the self-interested manipulation of masses by élites; the dangerous power of efficient bureaucracy; and for the creation of more humane conditions—the achievement of a new and appropriate community in society, of a satisfactory specialization within a meaningful whole, of justice in the whole fabric of institutions, etc. It was not only Marx, but also the great majority of these theorists, who saw the malaise of modern industrial capitalism, and who wanted not only to *understand* society, but also to *change* it; who saw the long perspectives of human struggle in the historical making of societies, and who now saw the possibility of the making of a new, more dignified, more humane society in which the achievements of civilization could at last be made available to all, and in which the dignity of the individual human person, as such, could at last be realized. They all saw the modern juncture as one in which man was responsible for making himself.

The making of sociology was still for the making of society. All its dimensions as a science had been richly developed. Its ethical commitments too remained unchanged. Its moral purposes were undiminished.

* Hobhouse. † Durkheim.